is one, however, that requires extended quantitative and qualitative treatment. The supposition that an action is *entirely* due to a colloid, because the action ceases on separation of the colloid from the system, is an error commonly made by physiologists, due to the omission of taking into account phenomena of adsorption, and the complete alteration of conditions produced by the change.

There can be no doubt as to the value of this work in its completed form; it traverses practically the whole of physiology in its chemical aspect, so far as it is now possible to do so, and illustrates in an excellent manner the results that have been produced through application of chemical methods to physiological problems; it is the first extended treatise of the biochemistry of plants, and as such fills a void that was distinctly appreciable, and moreover fills it in a manner that places all vegetal physiologists under great obligation to its author.

F. ESCOMBE.

EXPERIMENTS WITH EXPLOSIVES.

New Methods of Testing Explosives. By C. E. Bichel. Translated and edited by Axel Larsen. Pp. 62. (London: Chas. Griffin and Co., Ltd.) Price 6*s*. net.

IN collecting together and translating the papers on the researches carried out in the laboratory of the Carbonite Explosives Company, Hamburg, the translator has given to English readers a valuable and interesting little volume. The title is perhaps a trifle misleading, but the whole scope of the work may be seen from the following quotation:—"(1) Why does a smaller quantity of one explosive than another cause ignition of fire-damp? (2) What are the incidental phenomena and the influences tending to promote such result? (3) In what manner do they co-operate in producing it?"

Appreciating the fact that it is desirable to work with quantities as nearly as possible approaching those employed in actual practice, special apparatus has been constructed, so that for the particular explosives dealt with we now have details obtained from experiment on a much larger scale than any hitherto adopted. In some cases charges of gunpowder as great as 1500 grams were exploded, and for the higher explosives often 300 grams. Even for the calorimetric determinations the bomb had a capacity of 30 litres, which was capable of taking a charge of 100 grams. There must always, however, be some risk with heavy charges of recording undue pressures, a point to which Noble has directed attention.

The actual pressures, gas volumes and composition of the products were determined from charges fired in Bichel's apparatus, the pressure being recorded by a piston indicator working on a drum. The record is really in excess of the true pressure, but it is stated that the indicated pressure is rarely more than two or three per cent. from the actual. The apparatus permits of variations of surface area for a definite charge, and so the cooling effect of the chamber may be eliminated. It appears that with this allowance the pressure at a given density of loading is proportional to that with higher densities. This, however, may not be strictly true with very high densities.

The actual temperature at the moment of explosion was calculated from the heat developed, the composition of the products and their specific heat, in the usual manner, but all such calculations are uncertain owing to doubt as to the specific heats of gases at these high temperatures, and the impossibility of taking into account dissociation. The possibility of fitting a thermo-junction into the Bichel apparatus might be worthy of consideration, for although the results cannot approach actual values, yet the relative temperatures recorded would probably serve as a useful check on those calculated. Macnab has already employed the thermo-junction for this purpose.

In connection with the safety of explosives for mining, undoubtedly the length of the flame, its duration and temperature are of the greatest importance. The two former were recorded photographically, a quartz lens being used. Some excellent plates of the flames are reproduced. A factor deduced from the ratio of the flame duration to the detonation time, termed the "after-flame ratio," is shown to have the greatest influence on the ignition of fire-damp, and a most instructive diagram shows the temperature developed, the length of flame, and the "after-flame ratio" for the explosives examined.

In considering the efficiency of an explosive the author makes a distinction between the dynamic action, due to the projectile-like action of the products on the surrounding surfaces, and the static energy, deduced in the usual manner from the volume occupied by the products at the calculated temperature of explosion. It certainly seems that a more rational classification is thus possible than when the two are considered together, and the results are claimed to be fairly in accordance with those obtained in actual practice.

The general bearing of the work on the question of safety is clearly dealt with, and four very complete tables give a mass of information relating to the explosives examined.

Sufficient has been said of the contents of the book to convince those interested in the subject of its great value. It deals almost exclusively with mining explosives, but it would certainly be of very great interest if the investigations could be extended to military explosives, for the author has such valuable apparatus at his disposal that experiments on this large scale could not fail to give much valuable information.

J. S. S. B.

CAUSALITY AND THE HUMAN WILL.

The New Science of Causation. By H. Croft Hiller. Pp. xiii+386. (London: The Walter Scott Publishing Company, Ltd., 1905.) Price 10*s*. net.

IF intrepidity were the prime essential of a philosopher, this work would be epoch-making, and its author would be a thinker of the first rank. He claims to have formulated a case—to his mind, abso-

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lutely impregnable—against the intellectual foundations of empirical science. So impregnable, in fact, to the author's mind that he can afford to detail in the preface, with inimitable *naïveté,* his many discouragements in the preparation of the work—the fact, for instance, that of eight Fellows of the Royal Society with whom he has communicated all have declined to read and criticise advance proof-sheets. Sir Oliver Lodge has even gently indicated that Mr. Hiller's previous works have not impressed him; the letter is printed in full as a kind of *imprimatur.*

Briefly stated, Mr. Hiller's main position is that causality resides solely in the human will, and not at all in matter, atoms, ions, ether, electricity, or any of the other entities with which modern science deals. What reality ordinary objects possess is not quite clear, but apparently the action which we ordinarily suppose them to effect really belongs to the human being using them. Thus "a knife is a fetish facilitating cutting," *i.e.* cutting could quite well be done by the unequipped human will, but human nature being weak finds it useful for ordinary purposes to rely on the God-determined illusion of knives and scissors. Food becomes unnecessary, or can readily be replaced by poisons. In fact, there is no poison or disease at all but thinking, or rather willing, makes it so, *i.e.* if the individual will, acting on its own initiative, has not endowed an object with such and such attributes, then the consensus of other human wills, acting through hypnotic suggestion, so endows it. In this way, we presume, Mr. Hiller would account for the occasional death of infants by accidental poisoning. Doctors not only cure diseases, but also create and propagate them.

Considerations of space forbid a statement of Mr. Hiller's doctrine of perception, with its singularly elegant terminology—top storey of mind, mnemonic storey, and the like. But a word of criticism must be added, even if it is foolhardy to rush in where eight Fellows of the Royal Society have declined to tread. So far as we can understand our author, he seems in too great a hurry to explain abnormal experiences. He revels in things that make our flesh creep, people whose staple diet is strychnine, "Katie King" apparitions, ghosts that have pulses and heart-beats. Now of course we should all like to build up absolutely exhaustive systems, but at present well-sifted evidence of the extraordinary is so difficult to procure, and the abnormal is so often exploited by charlatanism for private ends, that science, which is long and patient, will rather wait a little and concentrate itself upon the normal. Again, there is obviously a difference in the glory of fetishes; there is one fetish which facilitates cutting, and another which facilitates Marconigrams. Will Mr. Hiller seriously maintain that a consensus of even all existing human wills could interchange these at its pleasure? Why had we to wait until the twentieth century for radio-activity? Could a sufficiently strong will in the nineteenth have produced the same effects by means of shoe-blacking?

We gather from the preface that this attempt to prove the rest of the world insane is merely a provisional instalment of a greater work, to be entitled "Sic Transit Scientia"! So important an effort to overthrow the walls of the empirical Jericho must be carefully timed; we can only suggest as the most fitting date of publication the eve of the Greek Kalends.

IONS AND ORGANISMS.

Studies in General Physiology. By Jacques Loeb. 2 vols. Pp. xxix+782. (London: T. Fisher Unwin, 1905.) Price 31*s.* 6*d.* net.

THE two volumes of papers collected under this title form one of the most interesting and suggestive works that have been published on the subject. The bold idea, that by means of alterations in the composition of the solutions that bathe the tissues it is possible profoundly to affect not only metabolism and growth, but also such processes as fertilisation, has led to a series of experiments here recorded that are well worthy of careful study.

The material with which Prof. Loeb and his pupils have worked has been in the main organisms of such a size that the whole animal could be acted on by changes in the salts dissolved in the water in which the animal lived. Most of the experiments were made with either the embryos or eggs of marine animals belonging to the groups of Annelidæ, Echinoderms and their allies; some were on fish embryos, some on hydroids, and the earlier experiments, which seem to have furnished the author with the leading idea for these researches, frog's muscle. This idea, shortly summarised, is that the changes in the composition of the solutions are effective on account of the properties of the various ions added or subtracted, and that by varying these one can control the various biological processes. The control is supposed to be direct, and ions are even termed "toxic" or "antitoxic" according to their suggested action on any process—for example, Sodium ions are "toxic," because they prevent the development of fundulus ova, Calcium ions are "antitoxic" because they neutralise this action.

The experiments which have perhaps attracted most attention are those on artificial fertilisation. Addition of HCl to the water in which the eggs of starfish (Asterias) were suspended caused them to develop parthenogenetically; similarly Ca was efficacious for the eggs of Amphitrite, KCl for Chaetopterus, and either KCl, NaCl, or even evaporation of sea water for the eggs of Arbacia, an Echinoderm. As to the accuracy of the observed phenomena, most of Prof. Loeb's readers will accept the evidence here adduced; whether the results bear the importance attached to them is a more open question. The author himself points out that these eggs are naturally on the brink of parthenogenetic development; in fact, if left to themselves they usually begin to segment spontaneously, and the effect of the addition of the various ions is only to hasten a naturally occurring process. It perhaps asks too much, but one regrets that the experimental difficulties so far seem to have prevented any of the parthenogenetic animals from attaining adult life.

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